To make the investigation exhaustive, an earnest appeal is herewith made to all persons who have instruments at their disposal to participate in the proposed observations and to put themselves in communication with the Division of Terrestrial Magnetism, U. S. Coast and Geodetic Survey, Washington, D. C., so that the necessary directions may be given them.

The scheme of work proposed embraces the following:

1. Simultaneous magnetic observations of any or all of the elements, according to instruments at the observer's disposal, every minute from May 17, 14 h. to 21 h., Greenwich mean astronomical time.*

[To insure the highest degree of accuracy attainable, the observer should begin work early enough to have everything in complete readiness in proper time.†]

2. At magnetic observatories all necessary precautions should be taken, so that the self-recording instruments will be in good operation, not only during the proposed interval, but also for some time before and after, and eye-readings should be taken in addition, wherever circumstances will permit.

3. Such meteorological observations as are possible for the observer should be made at convenient periods (as short as possible) throughout the interval. It is suggested that temperatures, *e. g.*, be read every fifth minute (directly after the magnetic reading for that minute).

4. Observations of atmospheric electricity and of earth currents should be made wherever possible, and any other phenomena of importance should be fully noted.

The request is made that observers send, as soon as possible, a full report of their work to the Superintendent of the Coast and Geodetic Survey.

L. A. BAUER.

*For America this interval occurs during the night hours of May 17th and 18th, civil dates, while for Europe, Asia, Africa and Australia, it occurs during the morning or afternoon hours (according to longitude of station) on May 18th, civil date.

†See directions followed by the Coast and Geodetic Survey during the recent eclipse, Journal 'Terrestrial Magnetism,' Vol. V., p. 143.

## CLAYTON'S ECLIPSE CYCLONE AND THE DIURNAL CYCLONES.

IN a letter with the above title, in SCIENCE of April 12th (N. S., Vol. XIII., p. 589), Professor Bigelow criticizes my papers on the 'Eclipse Cyclone and the Diurnal Cyclones.' His criticism is subdivided into two heads: (1) concerning 'some minor errors' which he thinks he finds in the formulas, and (2) concerning the theory of the cold-center cyclone.

My method of treating the winds observed during the eclipse was first to find the mean wind during the eclipse at each station, then to find the deflections of the observed wind from this mean at intervals during the eclipse, in order to see if there were any systematic changes which might reasonably be attributed to the eclipse. In getting the mean wind, I plotted the individual observations of direction and velocity, taking them as nearly as possible at regular intervals, and drew the resultant direction for each station. As a check on this method and for greater exactness I next computed the mean wind for stations where the wind directions were recorded to degrees of azimuth, or the observations were otherwise considered sufficiently accurate. Observations to only eight points of the compass are not in general sufficiently accurate to show the eclipse wind. The general winds were from the southwest near the central path of the eclipse of May 28, 1900, in our Southern States, and the observations with which I had to deal were something like the following:

| | Direction in degrees. | Velocity in miles. |
|---|---|---|
| (1) | S. 23° W. | 3 |
| (2) | S. 56° W. | 2 |
| (3) | S. 80° W. | 1 |

These were plotted as shown in diagram 1 *A*, in which the continuous arrows are the observed winds and the broken arrow is the mean wind. With the above values the plotted mean was found to be S. 44° W. Calling the observed wind $o$ and its velocity $v$, the mean was then computed as follows:

| | sin. $o$ | cos. $o$ | (sin. $o$)$v$ | (cos. $o$)$v$ |
|---|---|---|---|---|
| (1) | .391 | .921 | 1.173 | 2.763 |
| (2) | .829 | .559 | 1.658 | 1.118 |
| (3) | .985 | .174 | .985 | .174 |
| Sum. | | | 3.816 | 4.055 |

$$\begin{aligned} \text{log. } 3.816 &= 0.582 \\ \text{log. } 4.055 &= 0.608 \\ \hline \text{diff.} = \text{log. tan. } \theta &= 9.974 \\ \theta &= 43^\circ \end{aligned}$$

$\theta$ is the mean wind, and in computing it in the present case it was not necessary to consider the sign of the quadrant, as all were in the same quadrant. It is seen that the plotted mean and the computed mean agree within one degree, and agreements approximating to this I found throughout the work. Diagram 1 *B*, gives an example of another set of winds. These examples are only to illustrate the methods. In the actual computations there were from 10 to 30 observations. In my statement of the formula, Professor Bigelow thinks the $v$ should precede the sin. $o$ and cos. $o$, but why he attaches importance to this I do not know. The product of $xy$ is the same as that of $yx$. It is distinctly stated in my paper that $o$ is the observed direction and $v$ the observed velocity, so I fail to see how there could be any misunderstanding; but, if a change is needed, it would seem to me best to give the formula thus:

$$\text{tan. } \theta = \frac{\Sigma\,(v \text{ sin. } o)}{\Sigma\,(v \text{ cos. } o)}.$$

Having found the mean wind, my next step was to find the direction and velocity of the wind which, acting on the mean wind, would produce the observed wind in each case. This I sought to do by means of the well-known parallelogram of forces, as shown in Fig. 2. In this diagram, if *AB* represents the mean wind and *AC* the wind observed at some given time, then *AC* may be assumed to be deflected from the mean by a wind having the velocity and direction of *AD*. By plotting the observations in this way, I obtained the resultant winds for different portions of the eclipse area, and having plotted them, found evidence of a systematic circulation around the center of the eclipse area. This circulation followed the eclipse, and hence I conclude that it is caused by the eclipse. As before, I endeavored to check the plotted results and obtain greater accuracy by computing the resultant wind *AD* for certain of the stations. To do this, without changing the position of *AD*, I drew a line as represented by the dotted line *BD*, and from

FIG. 1.

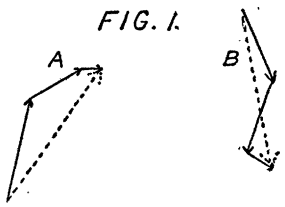



FIG. 2.

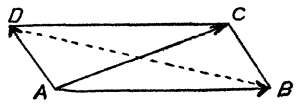



FIG. 3.

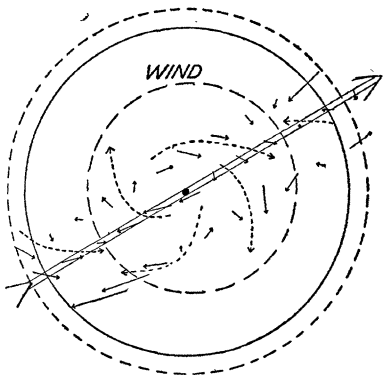
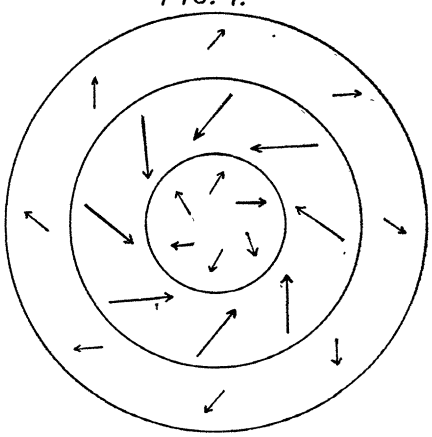



FIG. 4.



the triangle *ABD* proceeded to compute *AD* by well-known trigonometrical relations. I do not know why Professor Bigelow should find confusion because, in changing from plotting *AD* to computing *AD*, I changed the diagonal from *AC* to *BD*. I explained much of this in my paper, but, as I was writing for scientific men, it did not seem to me necessary to explain all the elementary processes.

Further details are given in the *Bulletin* of the Blue Hill Meteorological Observatory containing my discussion (*Annals* of the Astronomical Observatory of Harvard College, Vol. XLIII., Part 1, Observations and Investigations of the Blue Hill Meteorological Observatory, A. Lawrence Rotch, Director).

The final results as regards the wind circulation are illustrated in Fig. 3. In this diagram the arrows indicate, in the usual way, the direction of the resultant wind for different portions of the eclipse area, and the lengths of the arrows indicate the velocity of the wind. The central dark spot shows the area where the eclipse was total and the outer continuous circle shows the outer limit of the penumbra. The inner circle of broken lines shows the position of a probable ring of low pressure and the outer circle of broken lines shows the probable position of a ring of high pressure. The change of pressure found during the eclipse was in perfect accord with this latter conclusion derived from the wind circulation, and Professor Bigelow says that the observations of the Weather Bureau confirm my curve of pressure. The tendency to an anticyclonic circulation of the wind near the central area of the eclipse is confirmed by the observations of Professor Upton and Mr. Rotch in previous eclipses. (See *Amer. Meteor. Jour.*, Vol. IV., p. 456, Feb., 1888; also *Annals* of the Astronomical Observatory of Harvard College, Vol. XXIX., p. 18, 1893.)

I infer that Professor Bigelow does not seriously question the results as given by me, as he says Ferrel's 'cold-center cyclonic circulation * * * is just the opposite of the result of the eclipse observations.' It is the interpretation of the observations to which Professor Bigelow objects.

When I discover a new fact, it seems to me proper and even necessary for the advancement of science to consider the relation of the new fact to earlier known facts and to accepted theories and generalizations. In doing this I attempt to compare all the various facts and theories accessible to me and accept the best.

In considering the bearings of the eclipse data, I read among other things the views of Professor Bigelow in the International Cloud Report to which he refers. I find myself unable to accept his views of the air circulation in cyclones which he derives from the cloud observations, nor is his explanation of the diurnal change in pressure as an electromagnetic phenomenon satisfactory. However, in publishing results it is usually not possible for an author to enter into detail in regard to conclusions with which he is not in accord, nor is it best because it arouses strife and feeling. It seems best to give one's own conclusions and leave the future student to decide between conflicting views, as we have done.

In comparing my eclipse data with the different theories, I found that the results agreed so perfectly with certain views expressed by Ferrel concerning the cyclone with a cold center, that it leaves me no room for a new theory nor an improvement on his.

Ferrel says, "The conditions of a cyclone with a cold center which are the most nearly perfect are those furnished by each hemisphere of the globe, as divided by the equator, in which the pole is the cold center, and the temperature gradient from the pole toward the equator is somewhat symmetrical in all directions from the center. * * * The center of a cyclone with a cold center may, or may not, have a minimum pressure, according to circumstances. A certain amount of temperature gradient, and of pressure gradient which is independent of the gyratory motion, as explained in § 72 in the case of the general circulation of the atmosphere, is necessary to overcome the friction in the lower strata and to keep up the vertical circulation, upon which the cyclone depends; and the pressure gradient, which depends upon the temperature gradient and is independent of the gyrations, may be such that the increase of pressure in the central part due to this cause may be greater than the decrease of pressur

arising from the cyclonic gyrations, especially where surface friction is great" ('A Popular Treatise on the Winds,' pp. 338–339). This is from Ferrel's latest book, and note that he says the increased pressure 'is independent of the gyrations.' He explains that the velocity, $v$, of the air circulation tends to decrease the pressure at the center of the cyclone and not at the same time cause a rise of pressure, as is assumed by Professor Bigelow. The § 72 referred to reads as follows:

"The first effect of the motion in the upper strata of the atmosphere from the equator toward the pole, as in the case of the water flowing from the warmer toward the colder end of the canal, would be to fill up a little, as it were, the polar region with air from the equatorial, the effect of which is to increase the pressure a little in the former and to decrease it a little in the latter region, thus creating at the earth's surface and in the lower strata a gradient of pressure decreasing from the pole toward the equator, which would cause a counter current in the lower strata."

Ferrel does not state here whether the counter current would be anticyclonic or not, but states in other passages that such a gradient under the influence of the earth's rotation *would* produce an anticyclonic circulation. However, we are not left to inference in regard to Ferrel's views in this matter, for in the *American Journal of Science* (2 Ser., XXXI., p. 33, par. 15) he states his views explicitly as follows:

"Near the poles the tendency to flow toward the equator seems to be the greater, and causes a current there *from* the poles, which being deflected westward (5) causes a slight northeast wind in the north frigid zone, and a southeast wind in the south frigid zone. But this is only near the earth's surface, and the general tendency of the atmosphere in the upper regions must be toward the east as will be seen."

In the paper just quoted Ferrel gives a diagram of the air circulation in the polar cold-air cyclones which shows the circulation about the north pole as given in Fig. 4, in which I have made the north pole the center of the circulation for comparison with the eclipse circulation. The circulation around the pole indicated by Ferrel agrees with that shown by observations, as will be seen by comparing it with the carefully prepared charts in the 'Report on the Scientific Results of the Exploring Voyage of H. M. S. *Challenger*, 1873–76' (Vol. II.).

The similarity to the wind circulation found in the eclipse area must be apparent to every one (compare Figs. 3 and 4). The chief difference is that the anticyclonic circulation around the center, as compared with the surface inflow, is relatively larger in the eclipse circulation, and the outer circle of outblowing winds in the latter is only faintly indicated, owing to lack of observations. But the larger anticyclonic circulation around the center in the eclipse is exactly what theory would indicate for a circulation over a continental area where surface friction is great, and in a case where the gradient necessary to overcome the friction is feeble.

It may be asked why the diagram quoted here from Ferrel differs so radically from those given by Professor Bigelow. The explanation, I think, is simple. Ferrel, for the purpose of mathematical treatment, in the examples which Professor Bigelow quotes, was dealing with ideal cases in which there was little or no friction When his theory was applied to actual conditions on a globe possessing friction, his illustration of the cyclone with a cold center was like that I have given, with an increase of pressure and an anticyclonic circulation in the center at the earth's surface. The greater the friction the larger the central anticyclone must be to overcome the friction and maintain the circulation.

This view of Ferrel's theory is exactly that taken by Professor Davis, who is probably the ablest student of Ferrel in this country (*Quar. Jour. of the Roy. Meteor. Soc.*, April, 1899, p. 165).

The necessity of making every detail clear about the eclipse cyclone makes it necessary to omit consideration of the diurnal cyclones, but I think enough has been said to make clear the reasons for the contrasted views of myself and Professor Bigelow.

H. Helm Clayton.

Blue Hill Observatory,
April 16, 1901.